Dear Editor,

We would like to thank you and the Reviewers for the opportunity to improve our manuscript. We greatly appreciate the valuable feedback provided, which helped us clarify the objectives of our study and motivated us to conduct additional analyses. We carefully considered all the comments raised by the three referees, as already addressed in the responses previously posted in the open discussion.

In particular, the major revisions include:

- A thorough revision of the entire manuscript, with particular attention to the *Introduction* section, to more clearly state the objectives of our work and its position within the current state-of-the-art;
- Improvements to the *Methods* section, where we better describe the methodological approaches used and include the *Icemelt* component. Furthermore we have moved to the Appendix the method previously developed by *Pistocchi et al., 2017*, since that methodology showed worse results. We believe that this can also enhance the clarity and readability of the text;
- Additional analyses, including the evaluation of model performance with respect to topographical and climatic features, thus enhancing the value of using multiple basins across different contexts in this study. We also analysed performance variations across different months of the year and introduced an interesting analysis of the spatial similarity of river discharge at the pixel level to identify key differences between the two coefficients used;
- An expanded *Discussion* section, incorporating the points raised by the Reviewers—particularly the outcomes of a possible model recalibration following the post-replacement of the coefficient.

Although the main findings of the study remain unchanged, minor differences in the resulting metrics and plots may appear due to the model adjustment (specifically, the inclusion of the *icemelt* component) compared with the previous version.

We would like to thank you and the Reviewers once again for the valuable and constructive feedback, which has been carefully considered to improve the manuscript.

Sincerely,

Valentina Premier on behalf of all co-authors

Review paper egusphere-2025-2157

Assessing the Impact of Earth Observation Data-Driven Calibration of the Melting Coefficient on the LISFLOOD Snow Module

By Premier et al.

We thank the Anonymous Reviewer for providing valuable feedback. We have improved the manuscript according to his suggestions that allowed us to clarify several critical aspects that were not previously well explained. Below, we provide our point-by-point responses, highlighted in red, that refer to the manuscript changes.

General Comments

This study investigates the calibration of the snowmelt coefficient in the LISFLOOD hydrological model using Earth Observation (EO)-derived snow cover data. The authors propose two EO-based calibration methods and assess their impact on snow cover fraction (SCF), snow water equivalent (SWE), and discharge simulations across nine European river basins. The manuscript contributes to ongoing efforts to integrate remotely sensed data into large-scale hydrological modeling.

We thank the Reviewer for recognizing the topic importance.

However, several methodological ambiguities, design inconsistencies, and literature gaps limit the manuscript's clarity, reproducibility, and broader relevance. The introduction focuses heavily on LISFLOOD while neglecting to situate the work within the substantial body of existing literature on EO-based snow calibration and assimilation techniques, many of which have long addressed multi-objective calibration using snow and streamflow data. The authors should better articulate the novelty of their approach beyond its application to LISFLOOD.

We thank the Reviewer for the thoughtful and constructive comments. The *Introduction* has been thoroughly revised to improve clarity and conciseness. In the new version of the manuscript, we have reduced the space previously dedicated to the description of the LISFLOOD model and moved it to Section 2.2, which is dedicated to the model description. We have also clarified the research question and better positioned our work within the context of state-of-the-art studies employing multi-objective calibration (L25–L42 in the revised version). We believe that the main novelty of this study lies in assessing the effects of a more consistent representation of a specific LISFLOOD component—the snow module—on hydrological response. To this end, we calibrated the snowmelt coefficient using a novel Earth Observation (EO) dataset derived from high-resolution optical remote sensing data and re-ran the model with the EO-derived snowmelt coefficient, keeping all other settings consistent with the EFAS configuration. This approach is generalizable and can be applied to other hydrological models.

Below, we report the updated paragraphs that clearly articulate this idea and the overall aim of the study:

“Most hydrological models calibrate snow processes against streamflow observations. While effective for discharge simulation, this approach suffers from equifinality: multiple parameter sets can reproduce streamflow equally well but yield unrealistic representations of snow accumulation and melt (Beven, 2006, 2019). The problem is particularly pronounced in large-scale models, where uniform calibration across diverse catchments compounds structural uncertainties (Beven, 2012). As a result, calibration based solely on discharge is insufficient for reliable snow-related applications.

Earth observation (EO) data provide an opportunity to address this limitation. Over the past 15 years, multi-objective calibration using both streamflow and satellite-derived snow cover has become state-of-the-art (Tang and Lettenmaier, 2010; Franz and Karsten, 2013; Avanzi et al., 2020; Xiao et al., 2022; Zhu et al., 2024). However, whether such multi-objective calibration should remain the default, or whether alternative strategies can achieve a more realistic snow representation, remains an open methodological question.

In this study, we address this question using LISFLOOD, the distributed hydrological model underpinning Europe’s operational flood and drought monitoring systems (De Roo et al., 2000; Van Der Knijff et al., 2010; Burek et al., 2013). Specifically, we focus on its temperature-index snow module and the snowmelt coefficient (Cm), following earlier efforts done by (Asaoka and Kominami, 2013; Riboust et al., 2019; Gyawali and Bárdossy, 2022; Ruelland, 2024). We test whether Cm can be directly estimated from EO snow cover fraction (SCF) data after standard streamflow calibration, without re-calibrating the entire model. Using a novel high-resolution (50 m, daily) EO binary snow product (Premier et al., 2021) to derive SCF, we evaluate:i) how well the current LISFLOOD setup reproduces snow and streamflow; ii) whether EO-derived Cm improves snow process realism; and iii) the impact of such adjustments on hydrological performance across diverse European catchments.”

Our approach allowed us to directly evaluate the behavior of the snow module. Our analysis shows that adjusting the snowmelt coefficient to better represent the snow cover did not necessarily require recalibrating the other parameters. While the original model was already accurate for snow cover when considering the entire basin, this adjustment provided a more accurate agreement at the pixel scale when evaluating SCF. Crucially, these improvements in SCF did not alter significantly the model performance at the model calibration outlets.

We acknowledge that we did not develop an entirely new methodology, as many of the techniques used are established in the literature. However, we combined these techniques in a novel way to evaluate the LISFLOOD snow module differently from previous studies (e.g., Thirel et al., 2012; Pistocchi et al., 2017). Our approach includes a novel calibration technique made possible by the use of a newly developed, gap-filled high-resolution snow cover time series, which is expected to have higher accuracy than commonly used gap-filled Snow Cover Fraction (SCF) products. This is largely due to the integration of high-resolution data, which we elaborate on later in the answers.

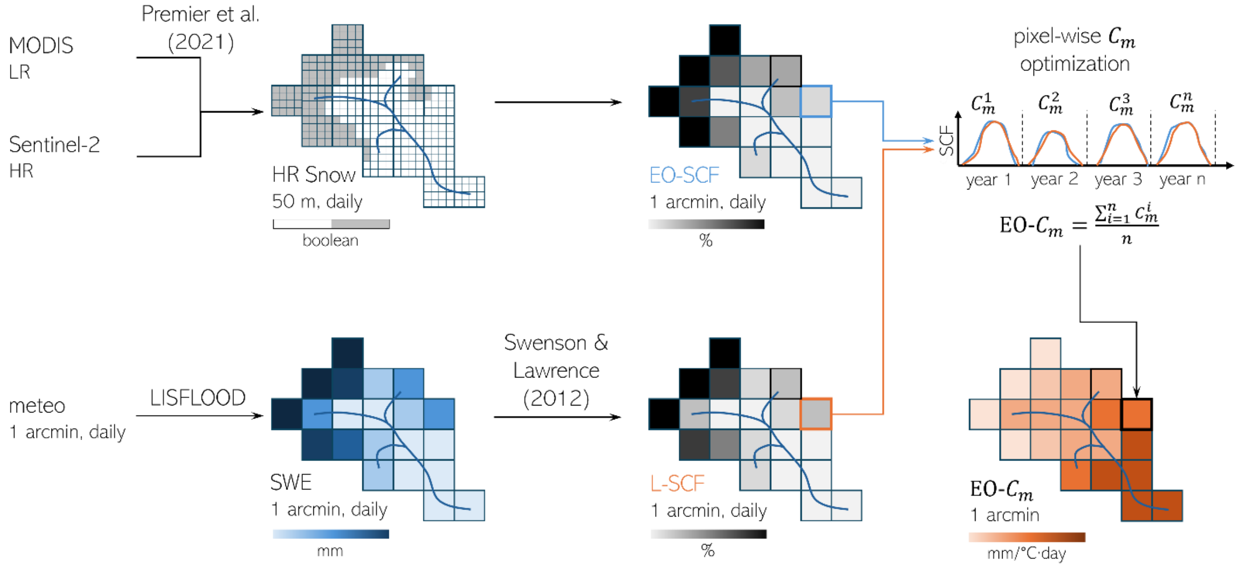


Figure 1 Workflow overview.

To further clarify the idea and methodology, we have added a graphical abstract and a preamble in Section 2 outlining the overall approach. We derive detailed daily Snow Cover Fraction (SCF) information from satellite data, which serves as a benchmark in our workflow. This is described in L60–66 of the revised manuscript.

"Daily high-resolution (50 m) binary snow cover data are used to derive SCF time series (hereafter EO-SCF; Sec. 2.1). In parallel, the LISFLOOD snow module (Sec. 2.2) is run to simulate SWE. A parameterization is then applied to transform SWE into modelled SCF (hereafter L-SCF; Sec. 2.3), enabling direct comparison with EO-SCF. The snowmelt coefficient (Cm) is treated as a free parameter of the snow module. We estimate Cm by minimizing the difference between L-SCF and EO-SCF through an optimization procedure (Sec. 2.4). For each year, we obtain a spatially-distributed coefficient (EO-Cm). A pixel-wise average over time is then computed. One season is excluded from calibration and used only for evaluation. In the final LISFLOOD re-run, the discharge-calibrated coefficient (L-Cm) is replaced by EO-Cm. "

This is particularly important given that the improvements achieved with EO-calibrated snowmelt coefficients remain modest, or even questionable, with respect to discharge simulations. This raises broader concerns about the hydrological value and operational significance of the proposed methodology.

We thank the Reviewer for raising this important point. Indeed, we recognize that the improvements in discharge simulations achieved through EO-calibrated snowmelt coefficients are modest and, in some cases, may appear limited. However, we can obtain a more accurate representation of the snow cover fraction (SCF). This said, we try to address a broader methodological question: *should multi-objective* *calibration (e.g., streamflow + snow cover) be pursued, or are alternative strategies that aim at a more realistic representation of snow cover feasible*?

Our findings suggest that a sequential calibration strategy—where the snowmelt coefficient is first adjusted upstream using EO-derived Snow Cover Fraction (SCF), followed by downstream calibration on streamflow—can serve as a viable and potentially more efficient alternative to full multi-objective calibration. In addition, we demonstrate that a standard calibration on streamflow, followed by a targeted post-adjustment of the snowmelt coefficient based on SCF, can still yield acceptable performance without the need for recalibrating the full model. We believe that this conclusion was not so obvious, as stated in L486-495 of the new *Discussion*:

"Our findings highlight notable differences in both the spatial distribution and the magnitude of Cm across the two calibration approaches. Despite these substantial variations, the EO-based coefficient primarily influences the timing of snow accumulation and melt phases. In some cases, this can affect the timing and magnitude of river discharge; however, differences in KGE and daily average discharge remain minimal in the catchments analyzed. We showed that EFAS5 and current calibration routine generally provide a good estimation on snowmelt dynamics, on average at catchment level. However, this conclusion was not obvious given the large number of calibrated parameters in LISFLOOD (14 in total) and the possibility for EFAS5 to have a parameter set that could reproduce well river discharge but not snow dynamics. A large number of parameters in distributed hydrological models can indeed results in a set of calibrated parameters that do not represent some key processes well, such as snow processes in this exercise, and instead exhibit compensation effects with other parameters in order to maximize the objective function (KGE) and the representation of a single process (river discharge)."

Moreover, several critical aspects of the methodology—such as the SWE–SCF parameterization, spatial resolution strategy, calibration procedure, and test basin selection—are poorly explained, inconsistently justified, or insufficiently analyzed. The comparison between models, methods, and performance metrics is often difficult to follow and underinterpreted. Key information for reproducibility (e.g., calibration configurations, data preprocessing protocols) is also lacking.

Many of the methodological components applied in this study (e.g., SWE–SCF parameterization, calibration procedures) are based on previously published and validated approaches. For the sake of brevity and to avoid unnecessary repetition, we opted to refer to those sources and keep the descriptions concise. However, following your comment, in the revised manuscript, we thoroughly revised Section 2 to make the methodological approach clearer, maintaining a balance between brevity and completeness. As a key improvement, we added a preamble and a workflow of the methodology (Figure 1). We also clarified the use of high-resolution (HR) data in the EO-based product. Section 2.2 was expanded to include subsections dedicated to the EFAS5 setup and the snow module. The section on snow cover parametrization was revised to make methodological details clearer, and Section 2.4 on snowmelt coefficient calibration was simplified by removing the previously developed methodology by

Pistocchi et al. (2017), which generally produced poorer results; this material has been moved to Appendix C to improve clarity and text flow. Finally, we added a new Section 2.5 titled "Assessment of the Results."

While the topic is relevant and the integration of EO data into hydrological modeling remains important, the manuscript in its current form suffers from fundamental methodological opacity, weak novelty positioning, and limited hydrological impact. Key sections are unclear or poorly justified, the experimental design is inconsistent, and the results do not support the claimed contributions. For these reasons, I recommend rejection. A revised version would require a substantial restructuring of both the methodology and the scientific framing to meet the standards expected for publication in HESS.

We thank the Reviewer for highlighting the weaknesses of the manuscript. We are confident that by addressing these points in the revised version, we have significantly improved the quality and clarity of the work.

Specific Comments

L14–15: This sentence is too vague to provide meaningful insight into the methodology or key contributions.

We thank the Reviewer for this comment. The sentence has been changed with "These findings highlight the potential of integrating EO data to calibrate the snow melt coefficient without changing other calibration parameters. This approach may offer practical advantages in situations that require accurate snow cover representation, although our results also show that standard calibration procedures provided an acceptable representation of snow dynamics". (L14-17)

L15–75: The introduction overfocuses on LISFLOOD and insufficiently addresses broader research on EO-based calibration and snow data assimilation. The authors should frame the novelty of their study in light of widely used multi-objective calibration approaches and explain how their work differs in terms of technique or purpose—not merely model specificity.

We thank the Reviewer for this comment. The description of LISFLOOD has been moved to Sec. 2.2 and only the most important details have been kept in the Itroduction. We would like to underline that the emphasis on LISFLOOD reflected the practical scope of our study—it is the tool available and relevant for our operational context. Although our experiments were conducted using LISFLOOD, we believe the rationale and calibration strategy explored here are applicable to other similar distributed hydrological models.

That said, the core objective of this work is to address a broader methodological question: *should multi-objective calibration (e.g., streamflow + snow cover) be pursued, or are alternative strategies feasible*?

Our findings suggest that a sequential calibration approach—first adjusting the snowmelt coefficient upstream using EO-derived SCF, followed by downstream calibration on streamflow—can be a viable and potentially more efficient alternative. Furthermore, we find that a standard calibration on streamflow alone, followed by a post replacement of the snowmelt coefficient based on EO-SCF, can still yield acceptable results without the need to recalibrate the entire model.

This has also been addressed in the Introduction (see previous answer to the Reviewer and L25–L42 in the revised version).

L59–60: The use of snow data in hydrological model calibration is not new and has become a common practice for over a decade. The statement should be revised to reflect this context.

Thanks for the comment. We removed the sentence and revised the Introduction. In detail, your comment is addressed in L30-34:

"Earth observation (EO) data provide an opportunity to address this limitation. Over the past 15 years, multi-objective calibration using both streamflow and satellite-derived snow cover has become state-of-the-art (Tang and Lettenmaier, 2010; Franz and Karsten, 2013; Avanzi et al., 2020; Xiao et al., 2022; Zhu et al., 2024). However, whether such multi-objective calibration should remain the default, or whether alternative strategies can achieve a more realistic snow representation, remains an open methodological question."

L66–67: The role of Sentinel-2 data here is unclear. If its main function is downscaling MODIS, this should be stated explicitly. Furthermore, cloud-free MODIS products (e.g., MOD10A1 Version 6) and well-documented gap-filling techniques are already available—please clarify why these were not used or compared.

We thank the Reviewer for the insightful comment, which allows us to clarify the role of high-resolution data in our methodology. While a simplified interpretation might suggest that Sentinel-2 data are used solely to downscale MODIS, this is not entirely accurate. As described in Premier et al. (2021), Sentinel-2 data are also employed to correct MODIS observations. Our method relies on the assumption that high-resolution (HR) data are more accurate than low-resolution (LR) data, particularly for fractional snow cover, where Sentinel-2 can detect snow patches that MODIS may miss.

Unlike other state-of-the-art approaches that only downscale MODIS while preserving its Snow Cover Fraction (SCF), our method also corrects SCF, offering a novel improvement. While we acknowledge the existence of various gap-filling and downscaling algorithms in literature, we consider the validation of our methodology beyond the scope of this paper, as it has already been published in Premier et al. (2021).

That said, we have revised Sec. 2.1 accordingly. We agree with the Reviewer on the importance of comparing our method with other existing and operational products. For this reason, Appendix A includes a comparison with the gap-filled VNP10A1F product, which demonstrates that it can serve as a good alternative to our more complex and labor-intensive algorithm. Although MOD10A1F could also be tested, we expect similar performance due to its comparable characteristics and gap-filling strategy. Additionally, VNP10A1F offers a slightly higher spatial resolution, which is advantageous. Hence, we think that our analysis is already exhaustive for the purposes of the paper.

L110–115: Calibrating 14 parameters without detailed justification seems excessive. A summary table of parameters and ranges is essential. A sensitivity analysis would help assess the importance of the snowmelt coefficient relative to other parameters and reveal possible interdependencies.

The table below specifies the 14 model parameters typically fitted in a LISFLOOD calibration. The model has more parameters (for instance, the temperature thresholds that define precipitation as snowfall or the start of snowmelt), but this is the selection of the most sensitive parameters after years of working with the model. Not all parameters are sensitive in all the catchments, e.g., the reservoir parameters are not in a catchment without reservoirs, or the snowmelt coefficient in areas where snowfall never occurs. These cases are identified by the calibration tool and the irrelevant model parameters are removed from the calibration. Further details can be found here: https://ec-jrc.github.io/lisflood-code/4_annex_parameters/ (reference also added in L112).

Table 1. Calibration model parameters in the OS-LISFLOOD model.

Process	Parameter	Range			Unit
		Minimum	Default	Maximum	
Snow	Snowmelt coefficient	2.5	4	6.5	mm/°C·day
Soil	Xinanjiang b	0.01	0.5	5	-
	Preferential flow	0.5	4	8	-
	Groundwater percolation	0.01	0.8	2	mm/day

	Upper GW zone constant	0.01	10	40	days
	Lower GW zone constant	40	100	1000	days
	Lower GW zone threshold	0	10	30	mm
	Groundwater loss	0	0	0.5	mm/day
Streamflow routing	Manning's n modifier: main channel	0.5	1	2	-
	Mainning's n modifier: floodplain	0.5	1	2	-
	Trigger of split routing	0	2	20	-
Reservoirs and lakes	Reservoir: normal storage	0.01	0.8	0.99	-
	Reservoir: normal release	0.25	1	2	-
	Lake multiplier	0.5	1	2	-

Regarding the purpose of this research, the independent calibration of the snowmelt coefficient does not interfere with the rest of the model parameters. On the contrary, that is what sequential calibration tries to avoid. In the "traditional" calibration, the snowmelt coefficient would be calibrated together with the other 13 parameters to streamflow. In that scenario, due to equifinality, it may happen that the calibrated snowmelt coefficient does not correctly reproduce snow processes, but the streamflow simulation performs well. That is actually what we wanted to explore with this study.

L111: Please clarify whether model calibration is based solely on streamflow using KGE. If so, this should be justified in light of the study's stated focus on snow processes.

The paragraph explained the usual LISFLOOD calibration (EFAS5 setup), where the snowmelt coefficient is fitted together with all the other parameters to maximize the Kling-Gupta Efficiency of the streamflow in a gauging station downstream. It is introduced here for reference, as a comparison with the sequential approach explored in the analysis. To make this clearer, we split the paragraph into two subsections, i.e. 2.2.1 "EFAS5 Setup" where we explain the details of the EFAS5 setup, and 2.2.2 LISFLOOD Snow Module that explains the snow module in detail, that is the objective of our work and the one affected by our EO-based calibration.

L123–124: If all 14 parameters are optimized per sub-basin against streamflow, why isolate the snow module for analysis? The risk of equifinality and parameter interactions should be acknowledged and discussed.

We thank the Reviewer for the comment and would like to clarify the calibration routine of LISFLOOD. The L-Cm snowmelt coefficient was calibrated as part of the LISFLOOD calibration for the European domain within the European Flood Awareness System (EFAS) and the European Drought Observatory (EDO). This calibration routine involves 14 parameters, and is based solely on streamflow using KGE, as detailed on the CEMS page:
https://confluence.ecmwf.int/display/CEMS/EFAS+v5.0+-+Calibration+Methodology+and+Data
In this study, LISFLOOD itself was not recalibrated. The evaluation of the module structure and the calibration routine were not within the scope of our work.

Given that the model calibration is based exclusively on streamflow and involves multiple parameters, it can lead to satisfactory streamflow reproduction but does not guarantee an accurate representation of snowmelt processes. This limitation may arise due to parameter interdependencies, model complexity, equifinality, and the common challenge in distributed hydrological modeling of "being right for the wrong reasons" (Beven and Cloke, 2012).
For this reason, we decided to calibrate the snowmelt coefficient using EO data and then re-run the model with only the EO-snowmelt coefficient different from the EFAS setup.
This process allowed us to evaluate how the snow module behaved.
What we found out is that by changing the Snowmelt coefficient, we observe a quasi-equifinal for streamflow but not as equifinal for SCA, SWE and melting. The catchment average discharge at the outlet has low sensitivity to the changes in the snowmelt coefficient, effects of the new parameters in upper basins are expected (as we will show later), but the snowmelt coefficient calculated using the traditional calibration compensates for local differences. If we work with a model where this holds true, we can calibrate the snowmelt coefficient to our will and then calibrate the rest independently, thus arguably achieving higher accuracy on snow but very similar on streamflow. We show that for LISFLOOD this holds true.
We acknowledge that sensitivity analyses have been conducted for LISFLOOD:
parameter uncertainty (https://www.tandfonline.com/doi/pdf/10.1623/hysj.53.2.293)
global sensitivity analysis (https://www.gdr-mascotnum.fr/media/mascot13zambrano-poster.pdf)
calibrated parameter analysis:
https://www.sciencedirect.com/science/article/pii/S0022169418307467?via%3Dihub
sensitivity analysis
(https://www.sciencedirect.com/science/article/pii/S2214581816300817?via%3Dihub)
global sensitivity and uncertainty analysis
(https://www.sciencedirect.com/science/article/pii/S0022169417301671)
While we agree with the Reviewer that a detailed sensitivity analysis would be valuable, especially in the context of LISFLOOD, we consider it beyond the scope of the current study, which focuses primarily on the snow module and the calibration of the snowmelt coefficient.

These points have been clarified throughout the manuscript – both in the Introduction and in the methodological Section, as well as in the Discussion.

L137: The description of elevation banding is unclear. How are elevation classes defined and implemented at the 1.4 km model resolution, which significantly smooths real terrain features? What are the implications for snow accumulation and melt representation?

We thank the Reviewer for the comment. We agree that our explanation of how elevation zones were defined was not sufficiently clear. Due to the relatively coarse resolution of LISFLOOD cells (1', ~1.4 km), significant sub-pixel variability in snow accumulation and melt can occur, particularly in areas with large elevation differences within a single pixel.
To address this, snow processes are modeled separately within three elevation zones defined at the sub-pixel level. These zones are determined based on a normal distribution of elevation values, which has been shown to represent well the actual distribution. To this purpose, the standard deviation of elevation within a grid cell is calculated from the Multi-Error-Removed Improved-Terrain (MERIT) DEM with a spatial resolution of 90 m. The three elevation zones—A, B, and C—are each assumed to cover one-third of the pixel area.
Assuming that the average pixel temperature corresponds to the mean pixel elevation, temperatures for the lower zone A and upper zone C zones are estimated by applying a fixed lapse rate (L = 0.0065 °C/m) to the elevation differences from the mean. Snow accumulation and melt are then modeled separately for each zone, using the temperature at each zone's centroid as a proxy for local conditions.
To improve clarity, we added these details in L148-154. They can also be found in the LISFLOOD model official documentation (https://ec-jrc.github.io/lisflood-model/2_04_stdLISFLOOD_snowmelt/)

L140–164: The SWE–SCF parameterization is central but confusing. Equations (7), (8), and (10) appear circular or contradictory. Their derivation, purpose, and assumptions must be clarified. Also, kaccum plays a key role but is not explained. A graphical illustration would help. The brief mention of the Swenson & Lawrence vs. Zaitchik & Rodell methods lacks depth and justification.

We thank the Reviewer for this comment, which lets us clarify the rationale and implementation of the SWE-SCF parametrization approach. First, regarding our choice of parameterizations, we are aware that several approaches have been proposed in literature. A non-exhaustive list that can be included in the manuscript is:

- Luce, C. H., Tarboton, D. G., & Cooley, K. R. (1999). Sub-grid parameterization of snow distribution for an energy and mass balance snow cover model. Hydrological Processes, 13(12-13), 1921-1933.
- Douville, H., Royer, J. F., & Mahfouf, J. F. (1995). A new snow parameterization for the Météo-France climate model: Part I: validation in stand-alone experiments. Climate Dynamics, 12(1), 21-35.
- Roesch, A., Wild, M., Gilgen, H., & Ohmura, A. (2001). A new snow cover fraction parametrization for the ECHAM4 GCM. Climate Dynamics, 17, 933-946.
- Liston, G. E. (2004). Representing subgrid snow cover heterogeneities in regional and global models. Journal of climate, 17(6), 1381-1397.
- Niu, G. Y., & Yang, Z. L. (2007). An observation-based formulation of snow cover fraction and its evaluation over large North American river basins. Journal of geophysical research: Atmospheres, 112(D21).
- Helbig, N., van Herwijnen, A., Magnusson, J., & Jonas, T. (2015). Fractional snow-covered area parameterization over complex topography. Hydrology and Earth System Sciences, 19(3), 1339-1351.
- Pimentel, R., Herrero, J., & Polo, M. J. (2017). Subgrid parameterization of snow distribution at a Mediterranean site using terrestrial photography. Hydrology and Earth System Sciences, 21(2), 805-820.

For completeness, some of the most (the most recent ones) have been added in L159-160.

While we acknowledge the importance of a comprehensive treatment of SCF parameterizations, this lies beyond the main scope of our study, which is to propose an alternative calibration approach for the snowmelt coefficient in the LISFLOOD model. To that end, we chose to test two parameterizations that offer a balance between model complexity and data availability. Swenson & Lawrence is also an approach widely used in the Community Land Model (CLM). On the other hand, Zaitchik & Rodell is a simpler empirical method requiring fewer input data, yet shown to produce consistent results. Our results (Table B1) show that both parameterizations yield similar performance in our experimental setup, with a general better agreement with the EO data when using Swenson & Lawrence.

Regarding the equations, they are derived from the mentioned publication and from the code of the CLM model, available here:
CTSM/src/biogeophys/SnowCoverFractionSwensonLawrence2012Mod.F90 at master · ESCOMP/CTSM

While we agree that we could better explain the meaning and role of the equations, we consider a full derivation of the SWE–SCF parameterization beyond the scope of this paper, as it would add considerable complexity and potentially distract from the main objectives. However, we provide here a brief conceptual explanation to help clarify the approach.

The accumulation formulation (Eq. 8 in the current version) is based on the probability that a pixel becomes snow-covered after a precipitation event. Specifically, the snow-covered fraction is defined as:

$$SCF = \min(1,\ k_{accum} \cdot SWE)$$

SCF corresponds to the probability that a pixel is snow-covered, with *kaccum* acting as a scaling parameter that relates SWE to fractional coverage. Accordingly, the probability that a pixel remains snow-free is 1 − SCF. For multiple independent snowfall events, the cumulative probability that a pixel remains snow-free is

$$SCF_{N+1} = 1 - \prod_{i=1}^{N}(1 - SCF_i)$$

Eq. 8 represents a more refined, probabilistic formulation for accumulation that uses a hyperbolic tangent function, which ensures that SCF asymptotically approaches 1 as SWE increases, to update SCF after each event.

Regarding the depletion curve (melting), Equation 9 now is derived empirically, as stated by the original authors. It is important to note that Equation 11 can be obtained by inverting Equation 9. Additionally, in the original paper, Equation 11 is reported with a typographical error; however, the correct formulation is implemented in the corresponding code. We have also been in contact with the original authors to confirm that we are using the correct version of the formula. Please, for deeper understanding check:

Swenson, S. C., & Lawrence, D. M. (2012). A new fractional snow-covered area parameterization for the Community Land Model and its effect on the surface energy balance. Journal of geophysical research: Atmospheres, 117(D21).

We have revised this section according to this answer.

We agree that, as stated in the original paper, the accumulation parameter kaccum plays an important role. For this reason, we chose not to keep it fixed (see L178). Instead, we calculate it dynamically using our EO-derived SCF data at the time of the first snow accumulation, as also suggested by the original authors. This parameter represents the ratio between SCF and SWE at the onset of accumulation—when the pixel is still only partially snow-covered—and is therefore essential for determining the rate or "speed" of snow accumulation. For completeness, we added the maps of the obtained kaccum coefficients in Appendix B, Figure B1.

L165: Is the snowmelt factor calibrated independently of other LISFLOOD parameters? If so, a discussion of the implications and potential benefits of multi-objective calibration (including SCA and runoff) is needed.

We thank the reviewer for this insightful comment. Indeed, the snowmelt coefficient was calibrated independently from the other LISFLOOD parameters, and no multi-objective calibration was performed in this study. The primary objective of comparing the EFAS setup with the same setup incorporating the independently calibrated snowmelt coefficient was to evaluate whether recalibrating this single parameter would significantly impact the hydrological cycle. Additionally, this approach enabled us to assess the robustness of the full LISFLOOD calibration—which involves 14 parameters—in accurately reproducing snow dynamics.
We acknowledge that this aspect was not clearly articulated in the manuscript. To address this, we added this in Section 2 (L 118-125). Furthermore, we expanded this point in the Discussion section (L496-502) where we discuss the benefits of a possible two-step calibration.

L169: The snow balance equation is invalid in glaciated basins where annual melt can exceed snow accumulation due to negative mass balances. The method should either exclude these basins or account for ice dynamics.

We thank the Reviewer for this valuable comment. The basins that include glaciers are: Adige, Alpenrhein, Arve, and Salzach. The basins without glacierized areas are: Gallego, Guadalfeo, Laborec, Mörrumsån, and Umeälven.

Although the glacier-covered area is relatively small in most of the glacierized basins—less than 1% of the total area (approximately 0.9% for Adige and Salzach, and 0.6% for Alpenrhein)—we acknowledge that glaciers can still have a non-negligible influence, particularly in the Arve basin, where the glacierized area is approximately 5%. We have added this information in L259-261.

We agree that the ice component has not been adequately addressed in EFAS5. Our initial intention was to mask out pixels where the glacier coverage exceeded a certain threshold during the calibration of the snowmelt coefficient. However, this would alter the EFAS5 setup that is the configuration that we want to test, wit all its limitations. Also from a remote sensing perspective, a proper representation of glaciated areas would require distinguishing between snow and ice surfaces and applying different coefficients accordingly. However, we believe this is beyond the scope of our work.

In this new version of the manuscript, we have integrated the icemelt component (Equation 8) to ensure full consistency with EFAS5. This component was not included in the previous version due to its limited influence. All results have been updated to account for this addition. In EFAS5, melt rates for ice-covered areas are adjusted using a sinusoidal function that increases melt during summer, reflecting enhanced radiation and changes in surface albedo. However, this remains a simplified representation that does not capture actual ice mass balance or dynamics. The updated results confirm that the inclusion of the icemelt component leads to negligible differences compared to the previous version without it.

L173–176: The intent of this paragraph is unclear. Please rephrase to clarify what is being estimated or illustrated.

Thank you for your comment. The purpose of the paragraph was to clarify the conditions under which Eq. 11 (old version) is valid . Specifically, this equation assumes a single, continuous snow period—defined as a sequence of days during which a pixel remains continuously snow-covered. In such cases, it is reasonable to assume that total accumulation equals total melt over the snow season, ignoring other processes like wind or gravitational snow transport.

However, in some pixels—especially at lower elevations or in temperate climates—multiple snow periods may occur (e.g., snow melts and re-accumulates later). In these cases, the Equation should ideally be applied separately to each distinct snow period. While this would be more accurate, it would also introduce additional methodological complexity.

For consistency with the original approach proposed by Pistocchi et al. (2017), we retain their simplification of applying the equation across all snow-covered days, regardless of whether snow cover is continuous or intermittent. This simplification is a known limitation of the method and one reason we expect improved performance from the optimization-based approach proposed in this study.

We rephrased the paragraph as follows: “Note that the equation is strictly valid only for a single, uninterrupted snow period, defined as a sequence of days during which a pixel remains continuously snow-covered. It is therefore not applicable to glacierized areas, regions with perennial snow cover, or locations with multiple snow cycles such as lower altitudes. However, we follow the approach of (Pistocchi et al., 2017) and apply the equation across all snow-covered days, regardless of continuity. The periods with snow presence are detected from EO-SCF. This simplification avoids additional complexity that would arise from segmenting and analyzing multiple snow periods per pixel.” (L648-653)

Furthermore, we moved all this part to Appendix C, given the worse performances of the method and to the simplifications that make the approach less rigorous w.r.t. the current proposed optimization approach.

L178: What is being compared here? A model simulation using observed SCFs? The terminology and structure are confusing and require clarification.

We thank the Reviewer for this comment. By L-SCF (LISFLOOD SCF), we refer to the snow cover fraction (SCF) estimated using the LISFLOOD model. The LISFLOOD model itself does not directly provide SCF as an output; instead, it outputs snow water equivalent (SWE), which is computed using Equations 1-3 (current version of the manuscript), as a function of the snowmelt coefficient Cm.

To derive SCF from the modelled SWE, a parameterization (Equations 8–9) must be applied. This parameterization derives SCF from SWE to SCF, thus being SCF a function of Cm too.

The snowmelt coefficient Cm is treated as a free parameter in our framework and is subject to optimization. To optimize it, we minimize the error between L-SCF and EO-SCF, which refers to the SCF derived from Earth Observation (EO) data and serves as a reference.

Apart from the deep revision of the manuscript, to clarify the terminology, we also added Table 1.

L165–191 (Section 2.4): This section should be rewritten to clearly explain both EO-based methods for estimating melt factors. The current text lacks transparency and methodological rigor.

Thanks for the comment. We have revised the Section and as major change, we moved to Appendix C titled “Alternative approach for snowmelt coefficient estimation” the methodology previously developed by Pistocchi et al., 2017. We hope that this clarifies the methodology adopted in our proposed approach.

The resolution mismatch between EO data (50–500 m) and model grid (1.4 km) introduces major issues. Downscaling MODIS to 50 m and then reaggregating to 1.4 km is not clearly justified. How are orographic gradients in precipitation and temperature accounted for at this coarse scale? The authors should better discuss whether a semi-distributed approach (e.g., elevation bands) or higher-resolution modeling would improve consistency with EO data and SWE estimates.

Thank you for the comment. As also mentioned in a previous response, our methodology is not straightforward downscaling of MODIS data. Instead, it includes a correction step aimed at addressing known limitations of the MODIS sensor, such as errors due to grain size variability, solar zenith angle, viewing geometry, and atmospheric effects that are not fully accounted for in the retrieval algorithm. Following the approach described in Premier et al. (2021), we do not treat the MODIS-derived snow cover fraction (SCF) as an absolute value. Instead, we interpret it within a “safety belt” of uncertainty and primarily rely on high-resolution data to reconstruct snow patterns through robust statistical analysis. These reconstructed patterns implicitly account for topographic effects, including orographic influences. While the snow cover retrieval method is not the main focus of this paper, we highlighted this aspect in the revised manuscript (L88-91).

Regarding orographic gradients, as noted in a previous response, our model accounts for elevation-dependent snow processes by dividing each pixel into three elevation zones. This semi-distributed approach allows us to represent variations in snow accumulation and melt processes with altitude. Also note that meteorological forcing (EMO-1) considers the temperature gradient with altitude. That's not the case for precipitation.

We agree, however, that higher-resolution modeling could improve consistency between model outputs and EO-derived SWE, particularly in complex terrain. Nevertheless, it is important to stress the fact that the current model has been developed to run at continental scale (resolution > 1km). Increasing the resolution would increase computational time and output data size, crucial considerations for a model that runs operationally. Increasing resolution does not guarantee improved performance in all model compartments, since some processes, now simplified or ignored, might become more relevant and higher resolution (Van Jaarsveld et al., 2025). Moreover, we would like to emphasize that fine-resolution modeling is only as accurate as the quality of the input forcing data. In many basins—especially at high elevations—observational data are scarce or of limited accuracy, which poses challenges for high-resolution modeling. In contrast, EO observations may capture some processes, such as wind redistribution of snow, more effectively when high-resolution acquisitions are available. Thus, the integration of EO data remains a valuable complement to physically based modeling.

The manuscript suggests the key research question is spatial calibration (pixel vs. basin scale, L47–48), but this is insufficiently explored. How do calibration results differ at each spatial scale? What is gained or lost?

Thank you for this important comment. As shown in Figures 3 and 4, the pixel-wise calibration might result in a distribution that highly differs from that of the lumped coefficients. Initially, our idea was to investigate whether snowmelt coefficients calibrated at pixel scale could reveal meaningful patterns or correlations with topographic, geographic, or land cover features. Our initial guess was that such correlations might eventually allow for snowmelt parameterization based on spatial characteristics alone—potentially reducing reliance on EO data, which are often labor-intensive to process. However, as shown in Table 3 and discussed in L288-298, our analysis did not reveal strong correlations between the calibrated snowmelt coefficients and those spatial features. We have moved this part from the Appendix to the main text, to highlight this discussion. We are also aware that while pixel-level calibration allows for more spatial detail and potentially better alignment with EO-derived patterns, it increases computational burden. Basin-scale calibration has shown in this study to be already sufficiently robust but may hide local heterogeneity. This has been addressed with the analysis on the Normalized Euclidean Distance (NED) – Figure 14 – that suggests caution when interpreting simulated discharge upstream of calibration stations. In these areas, discharge simulations using EO-derived Cm may diverge substantially from EFAS5 outputs.

L202–209 & Table 1: The basin selection lacks justification. Several catchments (e.g., Arve, Salzach) include glaciers, while others (e.g., Guadalfeo) are subject to strong anthropogenic influences (e.g., reservoirs, diversions). These factors are not modeled and introduce significant uncertainty. Their inclusion must be explained and justified—or their results treated with caution.

We thank the Reviewer for this comment. As already stated in the old Section 3, the basins were selected based on the following criteria: i) they are all snow-dominated catchments, ii) they represent a range of geographical contexts, iii) they cover diverse land cover types, and iv) they span a range of elevation zones. Additionally, as mentioned in L204 of the old manuscript, the initial selection also considered the availability of discharge data.

We acknowledge the Reviewer's point that some of the selected catchments, such as the Arve and Salzach (influenced by glacier melt), and Guadalfeo (subject to significant anthropogenic influence), include processes that introduce additional sources of uncertainty. In the specific case of the Guadalfeo River, the Rules reservoir—constructed in 2006, midway through our analysis period (1990–2020)—is included in EFAS. However, EFAS assumes the reservoir was present throughout the simulation period, which introduces bias in the model output for this catchment. Additionally, the Guadalfeo basin was not part of the EFAS calibration, which further contributes to the relatively poor model performance. These limitations were added in the revised discussion to help contextualize the results (e.g., L258-262, L404-409, and L448-452).
For a detailed explanation of the reservoir modeling approach in LISFLOOD, we refer the Reviewer to the official LISFLOOD documentation:
https://ec-jrc.github.io/lisflood-model/3_03_optLISFLOOD_reservoirs/
And for a comprehensive list of reservoirs included in EFAS, refer to:
https://hess.copernicus.org/articles/28/2991/2024/

Table 1 / Calibration vs. Regionalization: It is unclear why some basins are calibrated while others are regionalized. This methodological inconsistency needs to be explained. Consistent baseline comparisons are essential to interpret calibration effectiveness.

We thank the reviewer for the comment. Some basin parameters came from a regionalization approach and not calibration because of the lack of river discharge observation. The map of the domain calibrated with river discharge is presented here:
https://confluence.ecmwf.int/display/CEMS/EFAS+v5.0+-+Calibration+Methodology+and+Data

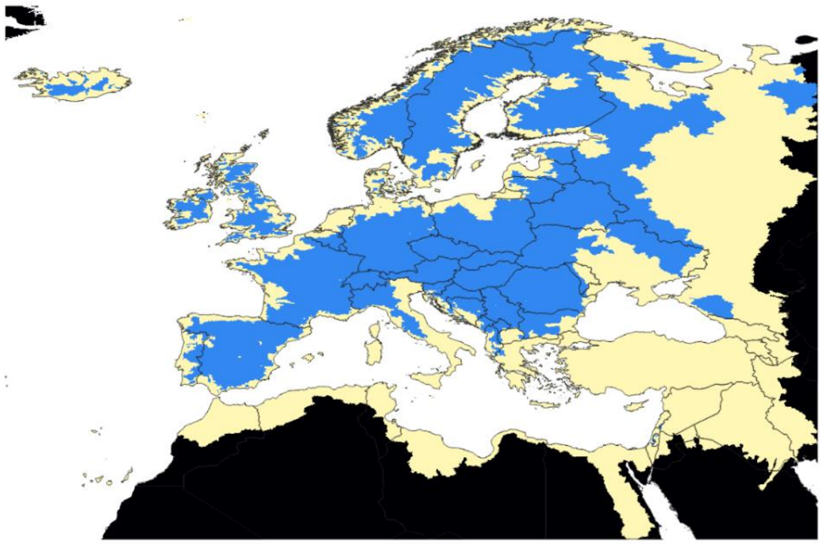


Figure 2 In blue the area of the pan-European domain for which discharge observations were available; in yellow the area of the pa-European domain for which discharge observations were NOT available for EFAS v5 calibration. The area in black is not included in the modeling domain.

The Adige and Guadalfeo basins were not calibrated during the calibration of the EFAS system; their parameters were assigned using the regionalization methodology from Beck et al. (2016) (see L113-116).
For this study, we managed to get observed data of river discharge, so the comparison against the simulated river discharge from LISFLOOD was possible.
As much as this could be seen as inconsistent, we believe that this was actually an opportunity to evaluate the regionalization approach effectiveness, a common challenge in data-scarce/ungauged basins. We stressed this better in L263-264.

Figure 1: The coarse DEM resolution leads to incorrect hypsometry (e.g., Arve basin's maximum elevation is ~4800 m a.s.l., not 3700 m). This smoothing likely affects snow (and ice) accumulation and melt modeling and should be discussed.

Thank you for the comment. We agree that the resolution is very coarse, but LISFLOOD has been developed as a large-scale model. However, as discussed previously in another answer, the intra-pixel variability is partially considered by using three elevation zones inside the pixel. Assuming that the average pixel temperature corresponds to the mean pixel elevation, temperatures for the lower zone A and upper zone C zones are estimated by applying a fixed lapse rate (L = 0.0065 °C/m) to the elevation differences from the mean. Snow accumulation and melt are then modeled separately for each zone, using the temperature at each zone's centroid as a proxy for local conditions.

Furthermore, according to what pointed out by Reviewer 3, we have added a clarification to caption of Figure 2 to avoid confusion by adding “The elevations reported here (and hence the color bar ranges) refer to the MERIT DEM aggregated at the model resolution of 1 arcmin.”

Regarding Table 2, we consider it more appropriate to report the elevation values that correspond to the minimum and maximum heights used in the model, based on the division into three elevation zones. Therefore, we define the minimum elevation as **$\min(DEM_i - \sigma_i)$** and the maximum as **$\max(DEM_i + \sigma_i)$**, reflecting the lower and upper bounds of the elevation ranges we model. We will updated both the table and its caption accordingly to clarify this approach

L204–209: The temporal alignment of model forcing (1992–2022) and snow data (2017–2023) is confusing. Are independent evaluation years used? If so, how is calibration/control separation ensured? A proper split-sample test would strengthen the study.

Thanks for the comment. The snowmelt coefficient has been calibrated over a five-season period, from October 1, 2017, to September 30, 2022. The sixth hydrological season, from October 1, 2022, to September 30, 2023, is used only for evaluation purposes. This period was chosen being the period of maximum availability of Sentinel 2 data (as stated already in L205, old version). After using the previous EO seasons to calibrate the snowmelt coefficient, we ran LISFLOOD in the period 1992-2023 to compare the effects of the differently calibrated snowmelt coefficients in the SWE climatology. We have included also the last seasons (2022/23) to have more consistent periods, however we this did not affect our outcomes.

L216 and Throughout: The manuscript uses many overlapping abbreviations for calibration methods (e.g., EO-Cm, EO-Cm1, EO-Cm2, LBFGS-B, L-Cm) with insufficient explanation. This confuses readers. Provide a summary table of methods and a glossary of acronyms. Terms should be redefined when introduced in different sections.

We thank the Reviewer for his comment. In a future version, we will add a summary table and redefine the terms in each section.

L220–225: The differences in results across basins should be discussed. Are certain physiographic features (e.g., elevation, land cover, glacier presence) associated with better or worse performance?

We thank the Reviewer for raising this interesting point, which inspired us to carry out an additional analysis. We added Figures 7 and 8 to the manuscript. The bias and RMSE are analyzed against selected physiographic features (mean elevation, forest coverage, and slope) and climatic features (mean precipitation, temperature, and snowfall). Furthermore, we distinguish basins with and without glaciers using different colors.

The results show a tendency for higher errors in lower-elevation and flatter catchments, while increased forest coverage is also associated with higher errors. Regarding the climatic features, an inverse relationship with RMSE is observed: basins with higher precipitation and snowfall tend to show lower errors. This is expected, since basins with less precipitation — especially less solid precipitation — have more ephemeral snow cover, leading to a higher fraction of partial snow cover and thus greater potential for errors.

Glacierized catchments do not appear to show substantial differences compared to non-glacierized ones. It is also noteworthy that the Umealven basin consistently stands out as an outlier with lower RMSE. This may be explained by its prolonged and complete snow cover, which likely results in more stable snow conditions and fewer errors.

These additional results were included in the manuscript in L340-351.

L270: This section is mischaracterized as a "water balance" analysis, but it is actually a comparison of LISFLOOD SWE with other model outputs. The full hydrological balance (precipitation, evapotranspiration, storage changes) is not analyzed, which would be relevant.

We thank the reviewer for the comment. We agree that the terminology is misleading since we are not reviewing the water balance of the model.
We created two separate subsections. The first one "4.3 Snow Water Intercomparison" covers the comparison against other models that estimate SWE, the other subsection called "4.4 Effects on Long-Term LISFLOOD Simulations" covers the comparison of the performance of EFAS5 run against the LISFLOOD results using the EO-Cm. Monthly averages were replaced with daily averages (Figure 12) for SWE, snowmelt and discharge.

Regarding the analysis of the other components of the model, our analysis focused on the impact of the snowmelt coefficient on discharge. Moreover, given the fact that LISFLOOD is a mass balance model, the effects on other components are limited, especially looking at the limited impact of EO-Cm on total runoff, shift (decrease) in infiltration/evapotranspiration are expected in Salzach, Arve, and Alpenrhein for the months of June-July given the higher runoff compared to the EFAS simulation.

The limited impact of the new snowmelt factor at catchment scale has been further analysed in the revised manuscript where we included an analysis that looks at the impact of the EO-Cm at sub-catchments level, and for upstream areas above 100 km2. The impact of EO-Cm is visible locally, and more precisely in some sub-catchments. This corroborates our thesis in saying that the current

EFAS calibration serves its purpose in representing well catchment average snowmelt dynamics. However, users should be careful when using simulated discharge upstream of the calibrated stations, since river discharge, in some cases, can be very different between the discharge from EFAS5 and the discharge computed using EO-Cm.
Those differences are shown in Figure 14 of the revised manuscript. We computed the Normalized Euclidian distance (NED) between the EFAS river discharge and the EO-snowmelt coefficient of river discharge. River discharge was selected for upstream areas above 100 km2 for both model outputs and min-max normalized. The NED was then computed between grid cells at the same location.
The spatially heterogeneous EO-snowmelt coefficients have a noticeable local impact in some river reaches with low upstream area, here the differences between model outputs are more pronounced. However, this influence decreases progressively downstream as localized effects are smoothed out along the flow path. By the time the discharge reaches calibration points, typically located further downstream, the impact becomes negligible, as the calibration process compensates for or overrides local parameter variations.
This is confirmed also by looking at the daily and monthly averages at catchment level, where differences between the two discharges are negligible, besides Salzach, Arve, and Alpenrhein. Therefore, while users can trust the current EFAS5 version to represent catchment-scale snowmelt-runoff dynamics, we recommend caution when interpreting simulated river discharge in upstream or mountainous areas, especially where inflow to reservoirs is critical, as local snowmelt-runoff processes may not be fully captured. These points have been included in the manuscript (L439-447 and L503-507).

L279–281: Comparing LISFLOOD SWE with other models without harmonized forcing data is misleading. The comparison should be framed as qualitative or exploratory—not as validation.

We completely agree with the reviewer that the analysis does not represent a formal validation. For clarity, we renamed the section as “Snow Water Equivalent Intercomparison”. We acknowledge that this analysis is not exhaustive and should be considered a preliminary step, especially given the lack of reference SWE datasets (remarked in L379-381). Even if in-situ SWE measurements were available, they would not provide a suitable reference due to the coarse resolution of the LISFLOOD model and the high intra-pixel variability introduced by complex topography. Additionally, other models cannot be considered absolute references, as they may have inherent limitations stemming from model parameterizations and the quality of forcing inputs.

Figure 7 and L319 etc.: “Climatology” is misused. Use “seasonal average” or “mean monthly values.” Also, explain what the envelopes in the figure represent. Monthly aggregation may obscure important daily dynamics—consider showing daily averages instead.

We agree with the reviewer, and we amended the terminology in the manuscript. We replaced monthly with daily averages (Figure 12).

Metrics Reporting: The interpretation of metrics (e.g., RMSE, KGE) lacks depth. What does a specific improvement mean in operational or hydrological terms? A summary table of relative improvements across basins would aid comparison.

We thank the reviewer for the comment. Given the limited differences between the 2 performances of the LISFLOOD model we do not believe that an extra table is necessary.
We included in the result a better description of the metrics. Bias is practically identical, which means that water is not stored nor lost between the two runs (L419). The correlation coefficient is slightly worse in some catchments, which means that the time of the peak flows is slightly hindered (L421-422). The variability has mixed outcomes (L424).
The decreased performance in correlation is the most significant in operational terms. A lower correlation coefficient suggests that the timing of peak flows is somewhat less accurately captured. This is particularly important in the operational context of EFAS5, where changes in correlation directly impact the system's ability to anticipate or delay peak flows, an aspect that is critical for effective flood hazard communication and early warning.
It should be stressed however that model outputs from the LISFLOOD run using the EO-snowmelt coefficient were not recalibrated. If the model were to be used operationally, it would undergo a calibration round where 13 parameters ought to be calibrated, the original ones (14) minus the snowmelt coefficient. After the re-calibration of the model, the new metrics should be analyzed in terms of their impact in operational terms.

L295–300: This methodological content appears out of place in the results section, indicating a need for clearer structure throughout.

We thank the Reviewer for the comment. We moved the paragraph to the Methodology section (L231-235).

L325–385: The discussion should better engage with existing literature on snow data assimilation. It is widely recognized that improvements in snow state representation do not always lead to improved streamflow prediction. This should be acknowledged and contextualized.
We thank the reviewer for the comment; however, our study does not assimilate the SWE state into the model. The SCF is converted into SWE and this value is used to calibrate the snowmelt

coefficient at pixel level, as further explained in the previous answers. We clarified this better in the Introduction (L25-42).

L345 & L380: The SWE–SCF conversion is treated superficially. Other formulations exist and should be discussed. Additionally, the distinction between calibration and data assimilation should be made clearer, especially if the authors position their method as a calibration approach.

Thank you for the comment. We have already provided additional details on the SWE–SCF parameterization in our previous response. In the revised manuscript, we better justify the choice of the Swenson and Lawrence, (2012) formulation (L161-164) and mention alternative approaches available in the literature (L159-160).

Regarding the distinction between calibration and data assimilation, we position our method as a calibration approach, since the snowmelt coefficient is statically optimized and kept constant across multiple hydrological years. That is, we use EO-SCF to calibrate a model parameter (the snowmelt coefficient), rather than dynamically adjusting the model state during runtime.

However, we also recognize that the methodology could be extended to a data assimilation framework, in which EO-SCF is assimilated in near real-time to update model states based on the observed EO-SCF. But this goes beyond the purpose of the manuscript.

Model Structure: The limited impact of improved melt factors on discharge suggests structural limitations in LISFLOOD (e.g., degree-day assumptions, decoupling of snow and runoff). These issues deserve more attention in the discussion.

We thank the reviewer for the comment.
The degree-day method is a very simple, conceptual approach that is broadly used in other large-scale hydrological models, such as PCRGlob, CWatM or mHM. Even though the model might benefit from an improved representation of the snow processes, we believe that, given the model scale and purpose, LISFLOOD is able to satisfactory capture the main processes. However, we mention in the Discussion the limitations of LISFLOOD and possible further improvements (L465-485).
Regarding the limited impact of the EO-cm on discharge, we believe that this is generally true when looking at the discharge at the station outlet (with some seasonal differences in Salzach, Arve, and Alpenrhein basins). As shown in the Normalized Euclidean Distance plots, local differences in discharge are present upstream.

L408–409: This conclusion is not strongly supported by the preceding results and should be revised or qualified.

We thank the reviewer for the comment. We agree with the comment, and we removed this sentence. Our study highlights that calibrating with SCF from EO can improve local dynamics, but that for the scale of the basin analyzed the impact on river discharge simulation is comparable with the parameters estimated by the traditional LISFLOOD calibration.

The manuscript would benefit from careful revision for clarity, structure, and language. Sections are often dense and overly technical, with insufficient explanation of key decisions. A clearer narrative structure, consistent terminology, and simplified figures would greatly improve readability.

We thank again the Reviewer for the meaningful comments that we have carefully addressed and have helped us to improve the quality of the manuscript. We have consistently changed most of the text and structure, simplified some parts and added the required analyses. Furthermore we have also revised the figures trying to make them clearer.

Review paper egusphere-2025-2157 (Reviewer Francesco Avanzi)

Assessing the Impact of Earth Observation Data-Driven Calibration of the Melting Coefficient on the LISFLOOD Snow Module

By Premier et al.

We thank Dr. Francesco Avanzi for providing valuable feedback. We believe that the manuscript has been improved by considering his suggestions and by clarifying several critical aspects that were not previously well explained. Below, we provide our point-by-point responses, highlighted in red.

Premier and colleagues have presented an improved calibration approach for the LISFLOOD snow module that is based on leveraging high resolution satellite data. This approach minimizes the error between observed and simulated snow cover fraction, and directly impacts the simulation of SWE. Authors test this method across a variety of study catchments in Europe and provide a detailed analysis of the improvements in snow simulation, as well as an interesting water balance perspective.

This paper is technically sound, and the effort of using high resolution satellite data into a large scale hydrological model is relevant and interesting for HESS. At the same time, there remain some aspects that could be expanded and improved. I still see value in this manuscript, and thus I am recommending a major revision.

We thank the Reviewer for recognizing the importance of the topic and for providing valuable feedbacks.

My main point is that using snow data in addition to streamflow data in calibrating a hydrologic model has been widely explored (see for example https://www.tandfonline.com/doi/abs/10.1080/01431161.2010.483493, https://www.sciencedirect.com/science/article/abs/pii/S0022169419312132?via%3Dihub, https://www.sciencedirect.com/science/article/abs/pii/S002216941300320X, https://hess.copernicus.org/articles/26/5627/2022/hess-26-5627-2022.html, https://www.sciencedirect.com/science/article/abs/pii/S0022169424013167). As a result of significant research in the hydrologic community over the last 15 years, a multi-objective calibration that involves at least snow and streamflow data is now considered state of the art. Meanwhile, even doing so does not necessarily imply an improvement in model performance.

We acknowledge that multi-objective calibration has been widely explored in the literature. In the revised manuscript, we better position our work within the context of state-of-the-art approaches, including citations to the references provided by the reviewer in the Introduction section (see L25-42).

As clarified in our response to Anonymous Reviewer 1, our work addresses the methodological question: should multi-objective calibration (e.g., streamflow + snow cover) be pursued, or are alternative strategies that aim for a more realistic representation of snow cover feasible?

Our findings suggest that a sequential calibration strategy—where the snowmelt coefficient is first calibrated upstream using EO-derived Snow Cover Fraction (SCF), followed by downstream calibration on streamflow—can offer a viable and potentially more efficient alternative to full multi-objective calibration. Furthermore, we show that a standard calibration based solely on streamflow, followed by a targeted post-adjustment of the snowmelt coefficient using SCF information, can still achieve acceptable performance without the need for recalibrating the full model.

This manuscript fits in this state of the art and confirms most of the conclusions above. To me, the most interesting points here are the use of high resolution satellite data and the inclusion of a variety of catchments, with different snow climatologies and various hydrologic characteristics.

Thanks for appreciating the work we have done in this direction. Using high resolution satellite data and a variety of catchments, we believe we have provided robust evidence for our claims about independent, ex post calibration of snowmelt coefficients.

In the revised manuscript, I would invest more effort in leveraging this variety of cathments as a way to draw process-based conclusions from this study that could allow for generalization: how are these catchments representative of specific snow climates? What do differences in results across these catchments tell us in terms of hydrological processes and the applicability of this approach in ungauged regions?

We thank Dr Avanzi for raising this key point. Our results indicate a dependency of model performance on catchment characteristics. As also discussed in our response to Reviewer 1, we observe differences in performance across basins depending on the climate/physiographic features. These results in terms of BIAS and RMSE for SCF derived from EO-Cm are reported in Fig. 7 and 8 of the revised manuscript. These performance metrics are evaluated against selected physiographic features (mean elevation, forest coverage, and slope) and climatic features (mean precipitation, temperature, and snowfall). Additionally, we distinguish glacierized and non-glacierized basins using different colours.

Our analysis reveals a trend of higher errors in lower-elevation and flatter catchments, and a similar increase in error with higher forest coverage. Regarding the climatic features, an inverse relationship with RMSE is observed: basins with higher precipitation and snowfall tend to exhibit lower errors. This is consistent with expectations, as lower precipitation—particularly solid precipitation—typically leads to more ephemeral snow cover, resulting in a greater proportion of fractional snow-covered areas, which are more prone to detection and modeling errors. Glacierized catchments do not appear to differ substantially in performance compared to non-glacierized ones. One notable exception is the Umealven basin, which consistently appears as an outlier with significantly lower RMSE. This may be due to its prolonged and near-complete snow cover throughout the season, which likely reduces snow cover variability and modeling errors. However, we acknowledge that it remains difficult to draw direct associations between specific climatic conditions and performance outcomes. These additional results were included in the manuscript in L340-351.

Regarding the applicability of the method to ungauged basins, note that both the Adige and Guadalfeo basins are modeled using a regionalization approach. Despite this, our results show that LISFLOOD performs acceptably in terms of SCF for these basins. The improvements introduced by the new snowmelt coefficient are comparable to those observed in the gauged basins, suggesting the method's robustness. However, as discussed in the original manuscript, the Guadalfeo basin shows significant underestimation of river discharge. This poor performance could be partially attributed to the limitations of parameter regionalization and/or model assumptions related to reservoir operations. Specifically, the Rules reservoir, which was opened in 2004, is included in the model throughout the entire simulation period, potentially introducing structural inconsistencies. While a more detailed assessment would require further investigation, we believe the proposed method is applicable to both gauged and ungauged basins. The improvements in snow representation appear to provide benefits in either case, particularly in enhancing the accuracy of the snow component without requiring full model recalibration. These considerations were added in the revised discussion to help contextualize the results (e.g., L258-262, L404-409, and L448-452).

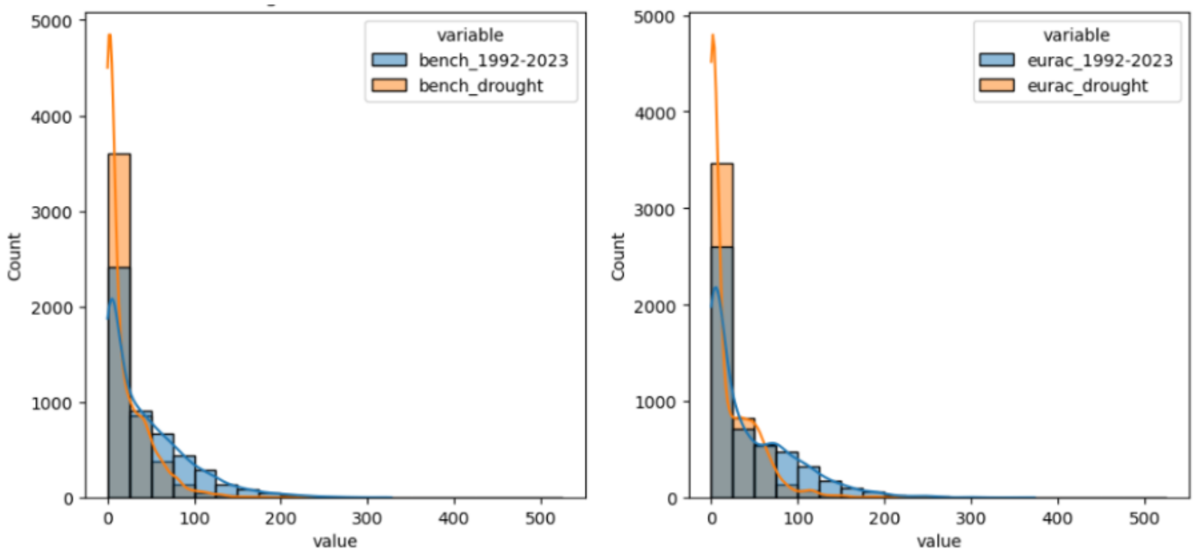
Authors also consider a fairly long period of data, with several snow drought episodes. Maybe commenting results across extremes and average years could be another way of bringing about more novelty.

We thank Dr Avanzi for this important suggestion. Keeping in mind that our study focuses on the calibration of the snowmelt coefficient, we believe that analyses on the broader capability of LISFLOOD to reproduce snow droughts or perform climatological analyses would be beyond the scope of this work and in our opinion were not the point of the Reviewer.

Our first idea was that a more detailed evaluation of snow drought representation would require the use of a temporally (or at least seasonally) varying Cm. This is because different characteristics that define snow droughts—such as thinner snowpacks, earlier onset of melt, and accelerated melt rates—are likely tied to different melt dynamics and would benefit from a seasonally adaptive calibration approach. In this study, we opted for a single optimized Cm averaged over five seasons, regardless of whether individual years experienced drought conditions or not. This is also discussed in L477-483.

Thus, we decided to focus on the Adige River basin and used the 2021/22 snow drought as a reference (the 2022/23 season falls outside the calibration period and could therefore bias our considerations). Contrary to our initial understanding, further analyses—now reflected in the revised manuscript (see Fig.5 and L286-287)—indicate an overall temporal stability of the snowmelt coefficient derived from our optimization routine. This suggests that, across seasons with contrasting hydrological conditions (from wet to dry), the distribution of the coefficient remains largely unaffected.

This finding is supported by Figure 10, which shows that snow drought events are well represented, and by Table D1, where model performance does not degrade during snow drought years. Therefore, we conclude that no marked differences are observed before and after the coefficient replacement. Additionally, we examined the spatial distribution of SWE under both average (30-year) and snow-drought conditions, but this analysis did not reveal any notable differences related to the coefficient change. Therefore, we did not include this figure in the revised manuscript.



The Introduction and the Discussion section could be revised to (1) make the scope of the manuscript broader and (2) discuss how this methodology compares to previous attempts in this realm (see above).

We thank Dr. Avanzi for the comment. We deeply revised the Introduction according to this and other reviewers' comments.

I agree with authors that a calibration in terms of snow cover fraction is currently the only feasible approach at these scales, even though this requires the additional complication of a SCF parametrization to convert modelled SWE into modelled SCF. This is well discussed in the manuscript, with the only recommendation of providing some additional results on the calibration of the k constants.

We thank the Reviewer for this comment. As described in the manuscript (L177-183) and following the approach suggested by Swenson et al. (2012), *k_accum* can be estimated by analyzing *SCF* and ΔSWE during at the time of the first precipitation event over an initially snow-free pixel. This is done by inverting Eq. 8 of the manuscript and by replacing with $SCF^n=0$. In line with our approach for estimating Cm, we computed kaccum at the pixel level for each of the five "calibration" seasons. The values were bounded to a maximum of 0.5, and pixels for which the coefficient could not be determined were assigned a default value of 0.1. The resulting seasonal values were then averaged over time to obtain a single representative constant.

For the sake of brevity, we did not include these results in the main text of manuscript, but they are presented in Appendix B in Fig. B1 for completeness.

In general, the manuscript is well written.

We thank Dr. Avanzi for his valuable feedbacks.

Review paper egusphere-2025-2157 (Anonymous Reviewer 3)

Assessing the Impact of Earth Observation Data-Driven Calibration of the Melting Coefficient on the LISFLOOD Snow Module

By Premier et al.

We thank the Anonymous Reviewer for providing valuable feedback. We believe that the manuscript has been improved by considering his/her suggestions and by clarifying several critical aspects that were not previously well explained. Below, we provide our point-by-point responses, highlighted in red.

Premier et al. present a calibration method for simulating snow melt using the LISFLOOD hydrological model. The authors tested the method on a sufficient number of basins in Europe and demonstrated the benefits for SCF, SWE, and to a lesser extent, for water balance components.

In my view, the paper's novelty lies in the use of improved high-resolution snow cover data, combined with an SWE to SCF approach and an optimization algorithm. It seems to me that the topic is relevant and of interest to the journal, but it requires a major revision.

We thank the Reviewer for recognizing the importance of the topic and the novelty of the work. We believe that the manuscript can highly benefit from the provided feedback.

The paper is missing some points:

It appears that the authors have introduced the SCF calibration as an alternative to the discharge calibration. Stepwise calibration or data assimilation using snow data, soil moisture, and evapotranspiration has been performed quite often in the last decade. Even with the Lisflood model attempts have been made by Thirel et al. (https://www.mdpi.com/2072-4292/5/11/5825, https://www.sciencedirect.com/science/article/pii/S0034425712003604).

We acknowledge that the novelty of this work does not lie in the use of stepwise calibration or data assimilation techniques, and we do not claim otherwise. We agree with the Reviewer that this point should be stated more explicitly in the revised version of the manuscript (L25-42). However, as also noted by the Reviewer, to our knowledge these approaches have not previously been applied using fully gap-filled snow cover fraction (SCF) data derived from high-resolution satellite imagery (at least, for LISFLOOD). We believe that the use of such a dataset represents a significant added value (L48-49).

The paper does not employ a multistep approach (first snow, second discharge) to improve overall calibration. As mentioned, Lisflood is the driving hydrological model of EFAS and GloFAS, which focus on discharge forecasting. An improvement in SCF is fine, but it cannot be the final goal. An improvement in SCF can even lead to a worse objective criterion of discharge, but might still be an improvement because it reduces the error of overfitting.

We agree that a multistep calibration approach would be a final goal for the full calibration of the model. This study focuses on the first step and the downstream consequences of calibrating only the snowmelt coefficient (Cm) without modifying the other parameters. Existing models typically generate SCF maps that are broadly correct but often lack details. We introduce a method to improve the fine-scale representation of these SCF maps. A key aspect of our work is that these improvements are achieved without affecting the mean SCA calculated at basin scale, which in turn preserves the discharge accuracy.

This is the reason why the analysed catchments do not show a significant decrease in KGE when the model was run with the new SMC, so in case of a new calibration we expect the equivalent KGE or an improved one. We will expand on this in the last comment. We also addressed this in the revised manuscript in Section 2 (L 118-125). Furthermore, we expanded this point in the Discussion section (L496-502) where we discuss the benefits of a possible two-step calibration.

However, given the importance of the 2 system at global and European level, a 2-step approach would have to go through an increased number on tests, and a full 2-step calibration approach, since the results in KGE could be different in other areas, as suggested by the reviewer.

Here, the focus is solely on the snow ablation process, using the snow melt coefficient (SMC) as the parameter. The process of snow accumulation, with parameters such as snow factor or temperature threshold that determine whether precipitation falls as rain or snow, is overlooked.

We thank the Reviewer for this insightful comment. We agree that our current focus on the snow ablation process, represented by the snow melt coefficient (SMC), overlooks important aspects of the snow accumulation phase. As acknowledged in L319-321 , we did not explicitly address parameters such as the temperature threshold that determines whether precipitation falls as rain or snow (e.g., *Tm*, usually set around 1 °C), or the snow factor.

We fully recognize the importance of accurately representing and tuning the snow accumulation process. In fact, errors in this phase may significantly impact the overall snowpack evolution. While such tuning can indeed be achieved by adjusting model parameters, we believe a major source of uncertainty may lie in the precipitation input data itself and specifically, in its amount, leading to incorrect accumulation regardless of model settings.

Additionally, we believe that relying solely on snow cover fraction (SCF) data may not be sufficient for constraining the accumulation process, particularly since precipitation events can occur over already snow-covered areas, where SCF remains unchanged. Therefore, while SCF is valuable, it may not provide enough information to fully and accurately calibrate accumulation-related parameters.

However, we believe that considering an average SMC over different hydrological years is also smoothing the effects of possible errors in precipitations. In any case, we acknowledge this as a limitation of the current study and plan to explore the accumulation process more thoroughly in future work. These considerations are added in L467-475.

The Lisflood snow modul is not explained fully. It is mentioned that Lisflood uses three different elevation zones (line 134), but it is not explained how the SCF calculation from SWE, the calculation of SMC (e.g., equation 12), or the optimization is performed with these three zones.

Thanks for the comment. We improved the LISFLOOD documentation (https://github.com/ec-jrc/lisflood-model/blob/jcr_revision/2_04_stdLISFLOOD_snowmelt/index.md) that will be published when LISFLOOD version 5.0 will be released. However, we did not consider the three elevation zones in the SCF parametrization, Eq. 12 or for the optimization approach. The three elevation zones play a role only in the SWE calculation, and in more detail in the temperature and consequently in the accumulation and ablation processes. Afterwards, a single SWE value per pixel is considered and the rest of the approach refer to the “average” SWE value for each pixel. We revised Section 2 accordingly.

Especially with higher resolution (here 1 arcmin) the day-degree approach can accumulate too much snow at high altitudes, as temperatures will not too often drop below 1° C. Lisflood uses a workaround to melt additional snow in Summer (IceMeltS). The paper does not mention this approach, nor does it take it into account.

Thank you for the comment. To partially address the issue that the degree-day approach can lead to excessive snow accumulation at higher elevations—especially when applied at higher spatial resolutions (such as 1 arcmin)—the use of three elevation zones was introduced. This zonal approach helps to mitigate overestimation of snow accumulation by better accounting for the altitudinal variation in temperature and snow dynamics. Regarding your comment on the Ice Melt integration, this contribution was neglected in the previous version of the work. It is now included in this version and all the results were replicated accordingly. See for details Sec 2.2.2.

The effect on the water balance is calculated on a monthly basis, even when the model is run on 6-hour timesteps. Here, it is really necessary to go on daily basis. With monthly evaluations, you miss the main advantage of your SCF calibration: having a better estimate of the timing of the main snow ablation, and therefore a more accurate estimate of the timing and magnitude of spring floods.

We thank the reviewer for the comment. We removed the monthly analysis and show the daily analysis (Figure 12 of the manuscript). The new results are commented in L385-415. We agree that that daily analysis is showing better differences in the timing of the peak. We also included the calculation of a seasonal KGE, showing the KGE and the relative metrics per season (Figure 13) together with the seasonal bias/RMSE for what concerns the SCF part (Figure 9). The results are commented in L428-438. Our analysis has highlighted negligible differences in terms of the overall metrics, however calculating metrics on a seasonally base might highlight a worse/better behaviour in the timing and magnitude of spring/summer floods.

The 2nd step of calibration for discharge is missing, as is an explanation of how to derive a better KGE with a change in SMC. In L_Cm version of Lisflood, SMC is calibrated to improve the KGE (SMC is optimized for discharge KGE). In the EO-Cm version, you changed only the SMC, and you keep all the other calibration parameters? The improvement in KGE (even the tiny one) can only be explained by a bigger range of SMC and/or by the single cell values. However, the calibration was performed on daily discharge; therefore, a comparison with daily values would be appropriate.

A 2nd discharge calibration is necessary to see the improvement vs the original calibration, using the new SMC as predefined values. I am not asking for all 9 basins but for those where you have only one subbasin (Arve, Laborec, Morrumsan, Umealven)

In the EO-Cm version, we modified only the Cm parameter while keeping all other calibration parameters unchanged. A larger range in Cm could lead to an improved KGE, depending on the catchment's sensitivity to snowmelt. Here, we report the average values over the catchment area for the two Cm settings used to run LISFLOOD. Except for the Alpenrhein, Arve, Salzach, and Umeälven catchments, the EO-Cm values are generally higher than the L-Cm ones, though they remain within the EFAS5 calibration range of 2.5–6.5. The Laborec, Mörrumsån, and lower Adige catchments are the only ones with average Cm values significantly above the 6.5 threshold. As pointed out in the manuscript (L281–285), these are areas where the EO-Cm optimization was not optimal due to fluctuations in snow cover.
The results in terms of KGE and average EO-Cm suggest that the overall catchment behaviour is similar. However, by increasing the range of EO-Cm, pixel-level dynamics can be better represented. Therefore, we argue that any potential KGE improvement after a two-step calibration would likely result from the combined effects of the broader coefficient range and the heterogeneity of EO-Cm. Simply increasing the Cm parameter

range in the EFAS5 calibration routine alone is unlikely to enhance performance and may even degrade the representation of SWE and snowmelt, as discussed in L496-502. Since we cannot demonstrate this directly, we have not included this consideration in the manuscript. However, we have added the average Cm values to the corresponding plots.

	Adige	Alpenrhein	Arve	Gállego	Guadalfeo	Laborec	Mörrumsån	Salzach	Umeälven
EO-Cm	5,4	2,2	2,6	6,6	5,9	9,1	9,2	3,4	2,9
L-Cm	3,6	3.1	2,9	4,4	4,1	4,5	6,5	4,2	3,0

We addressed the calibration comment in the specific comments below.

Specific comments:

- L2: I would not call it traditionally. It is not made because of tradition, but it has a reason. If you call it later traditional calibration, it is ok

  Thanks for pointing this out. We removed it throughout the manuscript.

- L21: This is unclear. It cannot be globally between 40-90% snow contribution from mountains. Please check Viviroli again

  Thank you for the comment. Our intention was to refer to *regional* contributions rather than *global* ones. This is clarified in the Introduction of Viviroli et al. (2007), which states: "regionally, mountain discharge may represent up to 95 percent of total flow in a catchment [Liniger et al., 1998]." This is further supported by Viviroli et al. (2004): "In humid areas, mountains supply up to 20–50% of total discharge, while in arid areas, mountains contribute from 50–90%, with extremes of over 95%."

  To avoid confusion, we will revise the sentence in the manuscript to read: "In mountainous and snow-dominated basins, snowmelt can account for 40–95% of annual discharge, depending on geography and climate (Viviroli et al, 2007)." (L21-22)

- L25: "LISFLOOD is one of the most comprehensive operational models used in Europe to simulate hydrological processes". This is very general sentence. Maybe a unique selling point: Lisflood is one of few operational models calibrated for Europe to simulate hydrological processes.

- We have deleted this sentence and revised the *Introduction* to place less emphasis on the LISFLOOD model itself and instead focus on how the study is positioned within the state-of-the-art literature and on clearly defining the aims of the work.

- L34: I think the equation which takes rain into account is from: Speers, D.D., Versteeg, J.D. (1979) Runoff forecasting for reservoir operations - the past and the future. In: Proceedings 52nd Western Snow Conference, 149-156

  We moved these details to Sec. 2.2.2 specifically L127-129.

- L65: for a “novel” method you explain not much in L183-184

  Thanks for the comment. We removed the word “novel”. As explained in L205, the optimization technique itself is not novel and it is part of the SciPy library. What we meant, is that this optimization approach has not been previously applied to LISFLOOD.

- L103: “The current model setup operates …”. Maybe put this after line 106, because the first part explains Lisflood, the second a special application of Lisflood for the EFAS setting

  We revised this paragraph.

- L131 it is rainfall per day not rainfall intensity. Somewhere else it should be hydrological year instead hydrological season

  Thanks. We replaced with the correct terminology.

- L136: The 3 zones can be explained in more detail and has to be included in 2.3 and 2.4.

  We thank the Reviewer for the comment. Also as pointed out by Reviewer 1, our explanation of how elevation zones were defined was not sufficiently clear. Due to the relatively coarse resolution of LISFLOOD cells (1’, ~1.4 km), significant sub-pixel variability in snow accumulation and melt can occur, particularly in areas with large elevation differences within a single pixel.

  To address this, snow processes are modeled separately within three elevation zones defined at the sub-pixel level. These zones are determined based on a normal distribution of elevation values, which has been shown to represent well the actual distribution. To this purpose, the standard deviation of elevation within a grid cell is calculated from the Multi-Error-Removed Improved-Terrain (MERIT) DEM with a spatial resolution of 90 m. The three elevation zones—A, B, and C—are each assumed to cover one-third of the pixel area.

  Assuming that the average pixel temperature corresponds to the mean pixel elevation, temperatures for the lower zone A and upper zone C zones are estimated by applying a fixed lapse rate (L = 0.0065 °C/m) to the elevation differences from the mean. Snow accumulation and melt are then modeled separately for each zone, using the temperature at each zone’s centroid as a proxy for local conditions.

To improve clarity, we adde these details that can be found in the LISFLOOD model official documentation (https://github.com/ec-jrc/lisflood-model/blob/jcr_revision/2_04_stdLISFLOOD_snowmelt/index.md) in L149-153. However, this does not affect the snow cover parametrization.

- Also the IceMelt part in https://github.com/ec-jrc/lisflood-code/ is not explained at all and not taken into account.

Thanks for the comment. We revised the documentation regarding the ice melt part at https://github.com/ec-jrc/lisflood-model/blob/jcr_revision/2_04_stdLISFLOOD_snowmelt/index.md, that will be published as soon as LISFLOOD version 5.0 is released.

At high altitudes, where the temperature never exceeds 1°C, the model accumulates snow as the temperature threshold for melting (Tmelt) is never exceeded. In these altitudes runoff from glacier melt is an important part. Snow will accumulate and convert into firn; then, firn is converted into ice and transported to the lower regions. This process can take decades or even hundreds of years. In the ablation area the ice is melted.

In LISFLOOD, this process is emulated by melting the ice in higher altitudes on an annual basis over summer.

$$IM_z = T_z \cdot C_{im} \cdot \Delta t$$

where:

- IMz is the icemelt (mm) per time step and elevation zone.
- Cim is the seasonally-varying icemelt coefficent (mm/°C day).

The seasonal icemelt coefficient enforces that icemelt only happens during summer (from June 13 to September 13 in the Northern Hemisphere, from December 13 to March 14 in the Southern Hemisphere). It also takes the shape of a sine function with a maximum value of 7mm/°C day:

$$C_{im} = \begin{cases} 7 \cdot \sin\left(\left(\text{doy} - \text{start} \right) \cdot \frac{\pi}{365.25} \right) & \text{if start} < \text{doy} < \text{end} \\ 0 & \text{else} \end{cases}$$

where start and end are the days of the year representing the beginning and end of the icemelting season, i.e., approximately summer.

However, in our first exercise we did not include this component since the glacier-covered area is relatively small in most of the glacierized basins—less than 1% of the total area (approximately 0.9% for Adige and Salzach, and 0.6% for Alpenrhein)—we acknowledge that glaciers can still have a non-negligible influence, particularly in the Arve basin, where the glacierized area is approximately 5%. Our initial intention was to mask out pixels where the glacier coverage exceeded a certain threshold during the calibration of the snowmelt coefficient. A proper representation of glaciated areas would require distinguishing between snow and ice surfaces and applying different coefficients accordingly. However, we believe this is beyond the scope of our work. The simplified approach as implemented in LISFLOOD has been integrated in the new version of the manuscript and all results updated accordingly.

- L146ff: This part can be done in a nicer way. E.g. https://egusphere.copernicus.org/preprints/2025/egusphere-2025-1214/egusphere-2025-1214.pdf has a better way to structure this.

  Thank you for the comment. We have reviewed the suggested paper, but we are not entirely certain about the specific concern raised—whether it relates primarily to stylistic presentation (e.g., adding a table with symbol definitions) or to conceptual clarity. Nevertheless, we revised the section to improve clarity. We also added a graphical abstract (Figure 1) together with a preamble that clarifies the methodology.

- L152: How is kaccum calculated?

  We thank the Reviewer for this comment. As described in the manuscript and following the approach suggested by Swenson et al. (2012), k_accum can be estimated by analyzing SCF and ΔSWE during at the time of the first precipitation event over an initially snow-free pixel. This is done by inverting Eq. 8 of the manuscript and by replacing with SCF^n=0. In line with our approach for estimating Cm, we computed k_accum at the pixel level for each of the five "calibration" seasons and then averaged these values over time to obtain a representative constant.
  The obtained kaccum maps are presented in Appendix B in Fig. B1 for completeness. The manuscript (L177-183) has been updated accordingly to this explanation.

- L155: What is the reference of this equation?

  All this part refers to the methodology proposed by Swenson and Lawrance (2012). More in detail, for the equations please refer to the code of the CLM model, available here:
  CTSM/src/biogeophys/SnowCoverFractionSwensonLawrence2012Mod.F90 at master · ESCOMP/CTSM

With respect to the original paper, there are some differences. In detail, Equation 11 is reported with a typographical error; however, the correct formulation is implemented in the corresponding code. We have also been in contact with the original authors to confirm that we are using the correct version of the formula. The text of Sec. 2.3 has been revised to improve clarity.

- L161: How is this calculated with the 3 elevation zones

  Thanks for the comment. We have explained in a previous answer how the elevation zones play a role in the computation of a different temperature. These zones are determined based on a normal distribution of elevation values, which has been shown to represent well the actual distribution. To this purpose, the standard deviation of elevation within a grid cell is calculated from the Multi-Error-Removed Improved-Terrain (MERIT) DEM with a spatial resolution of 90 m. The three elevation zones—A, B, and C—are each assumed to cover one-third of the pixel area. However, this does not affect the SCF parametrization, as explained in a previous answer.

- L169f: The equation 11 is invalid for glaciers and cannot applied for areas with always snow and with several snow-cycles. Equa 12 is again without the snow elevation zones.You showed it anyway, that this equation is not leading somewhere. It is fine to keep this approach.

  Thanks for the comment. We agree that this equation is a simplification, and it is not valid in the particular cases mentioned by the Reviewer. Given this and other limitations, and the general worse performances of this method, we decided to move this to Appendix C (L648-653). Anyway, both this Equation as well as the optimization approach, are not applied per each elevation zone. As stated in L147-153, the processes that are modeled separately for 3 separate elevation zones to take into account sub-pixel heterogeneity linked to elevation differences given the large pixel size are only the snow melt and accumulation.

- L183f: You featured this as "novel" approach. It appears to be an optimization for a single-parameter part of the standard SciPy library. Does seem to be novel and not explained at all.

  Thanks for the comment. You are absolutely right, the optimization technique itself is not novel at all and it is part of the SciPy library. What we meant, is that this optimization approach has not been previously applied to LISFLOOD and specifically to compute Cm, while previous work as the one of Pistocchi et al., 2017 has focused on simpler methodologies. On the other hand, other works as Thirel et al., 2012 or Thirel et al., 2013 have focused on a comparison or data assimilation. We removed the word "novel".

- Table1: the max elevation is not explained. In the original Merrit DEM it is much higher, so I assume you average that for 1 arcmin. But in the model the max elevation is the highest elevation zone. I think you should correct the elevation by the values from original Merrit DEM

  Thanks for the comment. Yes, you are absolutely right that this can be misleading and depending on the resolution of the used DEM, the values can be different. The DEM (also showed in Figure 2 of the manuscript) is the one aggregated at the LISFLOOD resolution of 1 arcmin. Therefore, we retained the figure and the color bar ranges as they currently are, as they correspond to the 1 arcmin DEM. Hence, we clarified this in the figure caption to avoid confusion by adding "The elevations reported here (and hence the color bar ranges) refer to the MERIT DEM aggregated at the model resolution of 1 arcmin."

  Regarding Table 2 of the new version, we consider it more appropriate to report the elevation values that correspond to the minimum and maximum heights used in the model, based on the division into three elevation zones. Therefore, we define the minimum elevation as **$\min(DEM_i - \sigma_i)$** and the maximum as **$\max(DEM_i + \sigma_i)$**, reflecting the lower and upper bounds of the elevation ranges we model. We updated both the table and its caption accordingly to clarify this approach.

- L297ff: Why using a monthly comparison. The original model is calibrated on daily values. The biggest effect of your improvents are the timing in days of the biggest drop in snow accumulation. The comparison to daily discharge is necessary to conclude if the models performance is improved (maybe the over discharge KGE is reduced, but some spring floods are better timed, the snow cover is better estimated).

  We thank the reviewer for the comment we replaced monthly with daily average discharge over years, as shown in Fig. 12.

  We made some preliminary analysis on peak timing of discharge; however, it was difficult to have drawn conclusions looking at single events. In some cases, peak was better captured in some other events no, in other events no difference was noticeable etc., this can be explained with the fact that are other factors contributing to the discharge at the location of the observed river discharge. The results are shown in the following figures but are not included in the manuscript.

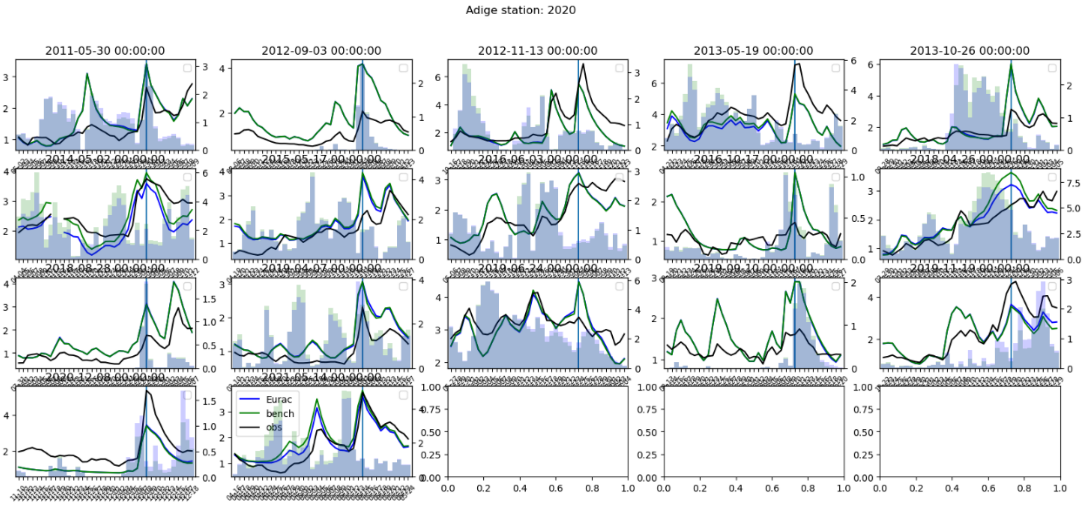


Figure 1 Peak events for the Adige basin.

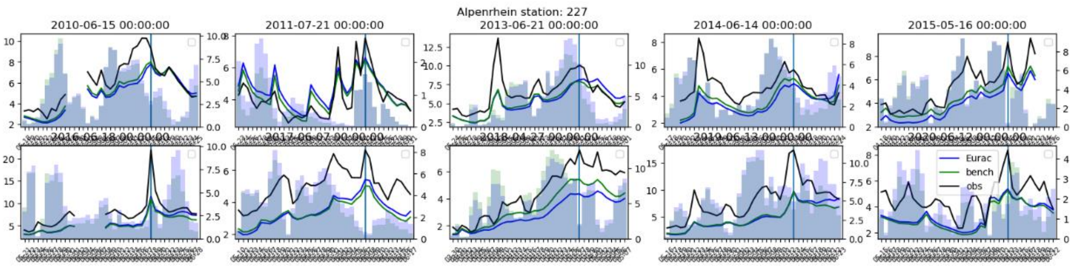


Figure 2 Peak events for the Alpenrhein basin.

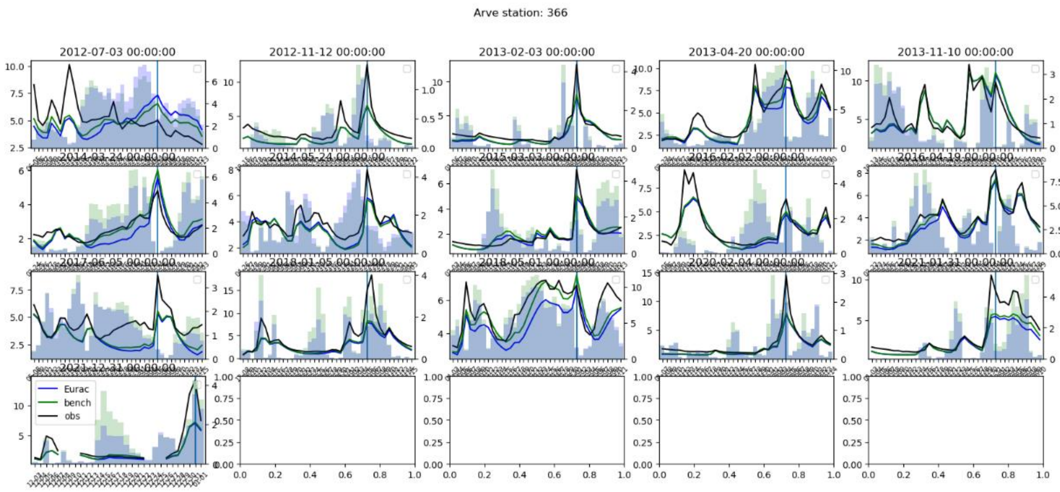


Figure 3 Peak events for the Arve basin.

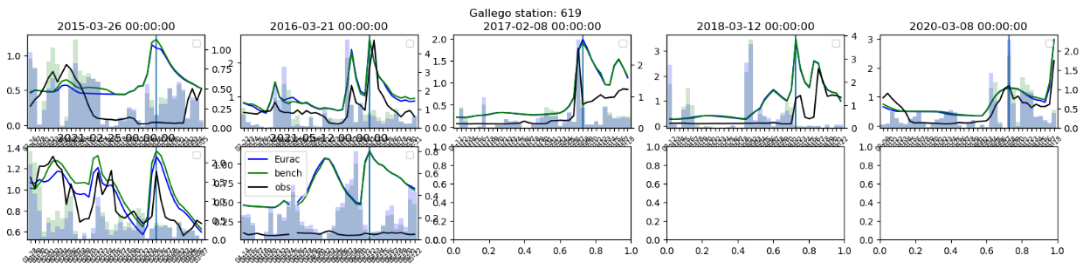


Figure 4 Peak events for the Gallego basin.

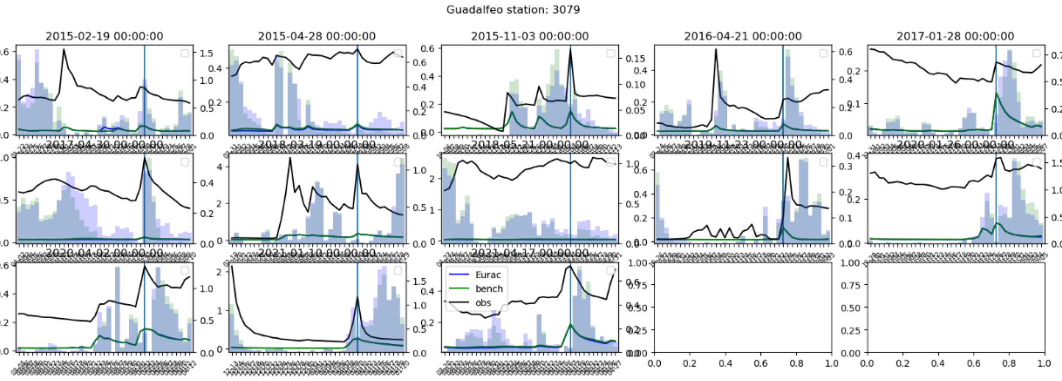


Figure 5 Peak events for the Guadalfeo basin.

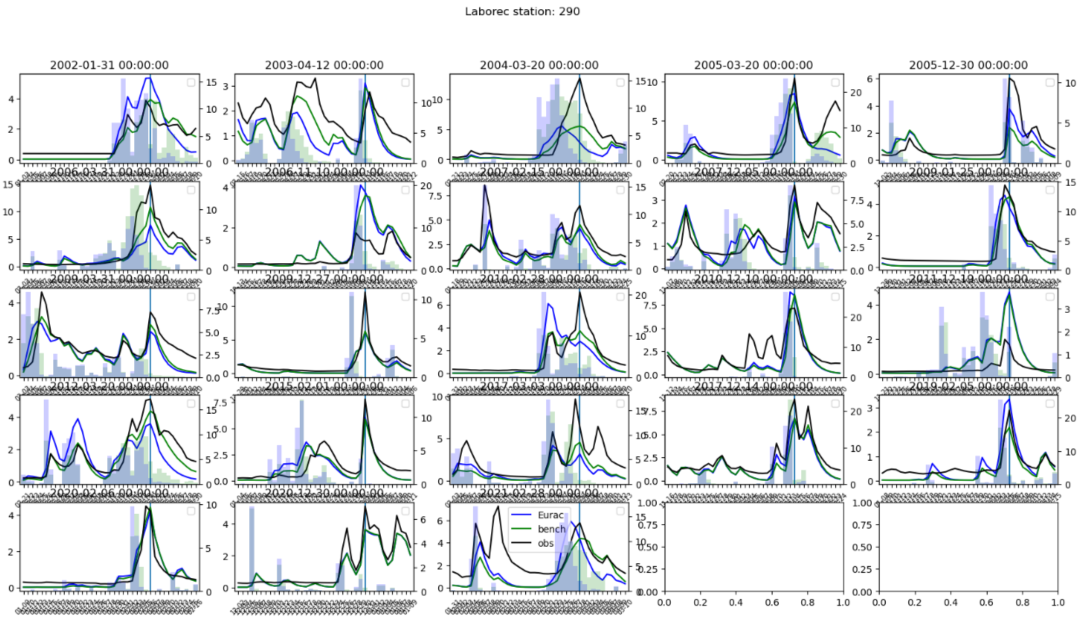


Figure 6 Peak events for the Laborec basin.

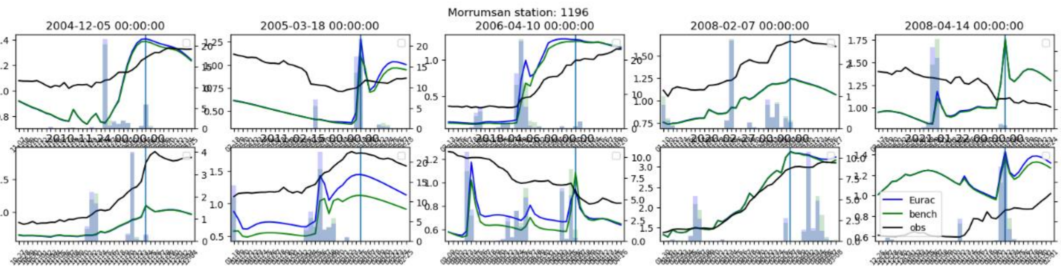


Figure 7 Peak events for the Morrumsan basin.

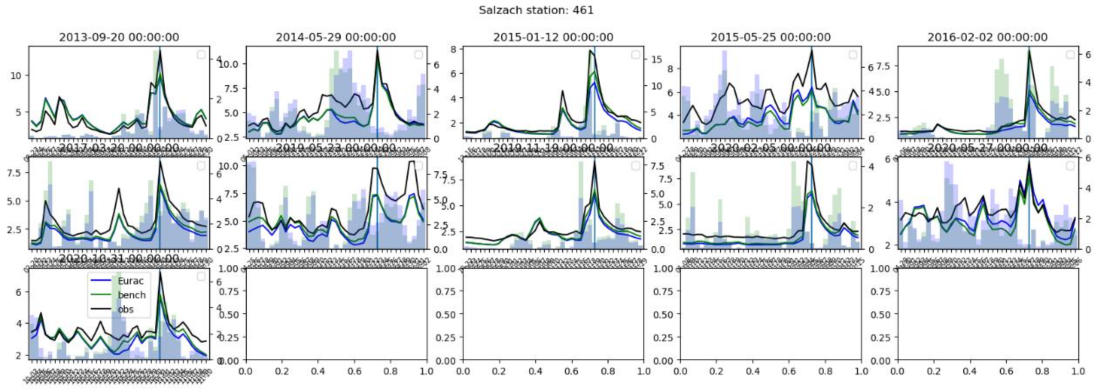


Figure 8 Peak events for the Salzach basin.

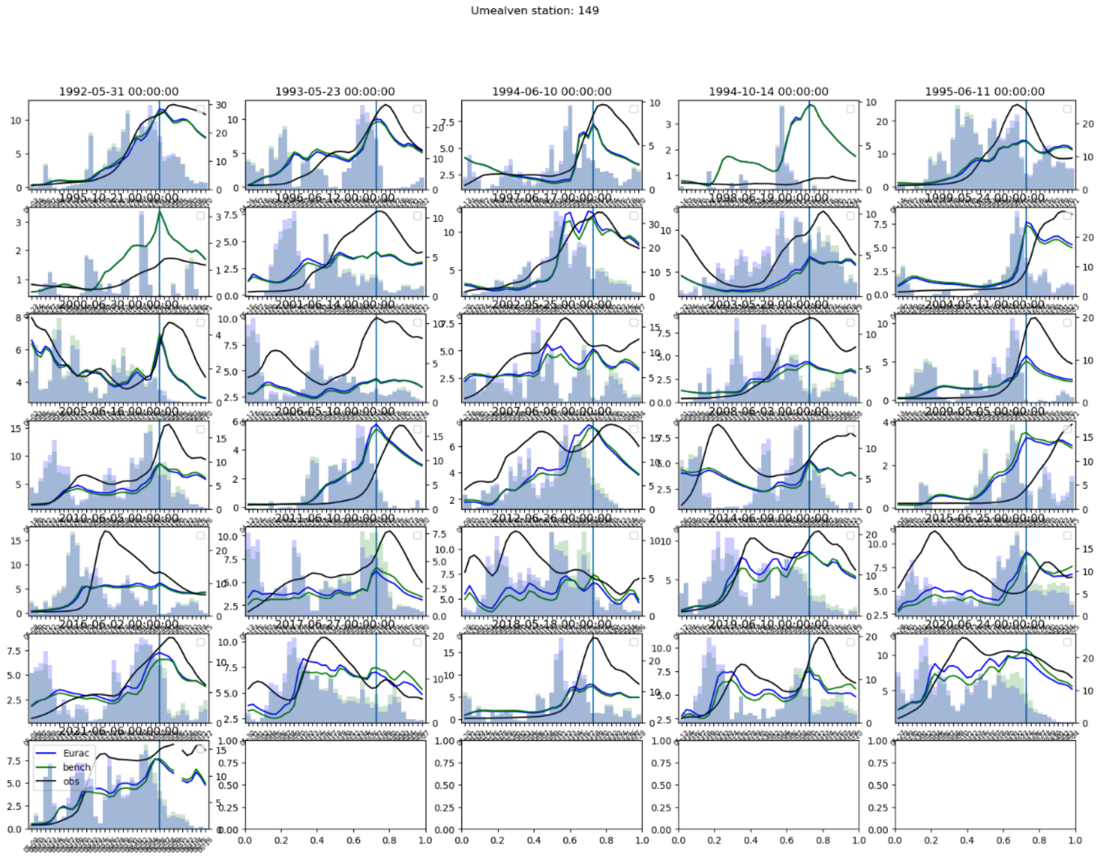


Figure 9 Peak events for the Umealven basin.

Another possible analysis that has been added in the new version of the manuscript is a seasonal KGE (see Figure 13).

- Fig 7: Gallego and Guadelfeo have some reservoirs included. It would be better to use subbasins without too much human interference. From the results, you cannot see if it is the snow or the reservoirs. You explained why Guadalfeo has a bad KGE. One solution could be to use only those years without reservoirs, The observed discharge for Guadalfeo river starts from 2014, so the bad performance is due the regionalized parameterization. We comment on this in L404-409.

We agree with the reviewer that ideally the less human influenced the better, however LISFLOOD is calibrated on human influenced streamflow we wanted to include catchments with different characteristics, as stated in L263-264.

In the case of the Gallego river, the reservoir (highlighted in orange in Figure) might influence partially a change in discharge as shown in the Figure 1.

- Table 4: This is not suitable for comparison. 1) you keep the other parameter constant (I assume, it is missing in the paper) 2) you compare on monthly values 3) you did not recalibrated the other parameters after setting SMC to your values.

  Thanks for the insightful comment.

  1) Correct. We described this better in the revised manuscript.
  2) No metrics are calculated on daily values of river discharge
  3) Correct.

- I think it is necessary to re-calibrate for a number of basins (maybe only those where you do not have upstream-downstream calibration) and discuss the effect of your improved SMC e.g. worse KGE but better representation of snow, more exact timing of snow-induced flooding,

  We thank the reviewer for the useful comment. We compared the simulation of streamflow with the new and old SMC with all other parameters unchanged, and the differences are most of the times very small or even negligible, the biggest difference in KGE takes place in the Laborec catchment (difference = 0.05)

  Given the fact that the calibration maximises the objective function (KGE), by recalibrating the model we do not expect a lower KGE compared to the one obtained by running LISFLOOD with the new SMC and the current LISFLOOD parameters.

  So, this reinforces three arguments that we have stressed better in the revised manuscript:

  - A possible 2 step calibration (1. Snowmelt coefficient on EO 2. remaining parameters), has the potential to improve the KGE. In the catchments we analysed, the KGE was slightly degraded in some cases, but within a narrow margin.
  - This procedure allows the integration of more realism on the snow component of the LISFLOOD model without recalibrating the other parameters. This may be very useful when in need to use this large-scale

model for specific purposes in snow-dominated catchments, while preserving consistency with the overall dynamics at the larger scale.

- The LISFLOOD model as currently calibrated and implemented for EFAS 5, can capture the average snow dynamics at the calibration station. Snowmelt dynamics are likely to change (as shown in the Euclidean distance figures) upstream, so river discharge affected by snowmelt dynamics should be treated with cautions upstream the calibrated stations.

We acknowledge that these considerations were not clearly articulated in the manuscript. To address them, we added revised Section 2 (L 118-125). Furthermore, we expanded these points in the Discussion section (L496-502) where we discuss the benefits of a possible two-step calibration.

Overall, the topic is interesting, and the potential for a good paper is there, but it lacks structure, and fundamental key points are not included yet.

We thank again the Reviewer for the meaningful comments that we have carefully addressed.